July 14, 2009

Physics Advisory Committee Meeting
June 23-27, 2009
Comments and Recommendations

Overview

The Fermilab Physics Advisory Committee (PAC) discussed some of the current, near-term, and possible future Laboratory programs. At the present time the Fermilab physics program is thriving – with exciting physics results from the collider experiments (CDF and DZero), from the neutrino experiments (SciBooNE, MiniBooNE, MINOS, and ArgoNeuT), and from experiments at the cosmic frontier (the Pierre Auger Observatory, CDMS, COUPP, and GammeV). The accelerator complex continues to perform superbly.

The mid-term program includes a significant participation in CMS, and the ongoing Fermilab neutrino program (MiniBooNE, MINOS, MINERνA, NOνA, and MicroBooNE), along with a Drell-Yan experiment and the mu2e experiment. The astrophysics program will be centered around DES, SuperCDMS, COUPP and the Pierre Auger Observatory, along with a number of smaller experiments. Both the physics and accelerator efforts of the long-baseline neutrino experiment and Project X will be major activities at the Laboratory. All of this together will provide a vibrant physics program at Fermilab in the coming decade.

Reclaiming the Energy Frontier

The Tevatron is operating at the current energy frontier. The Committee commends the Laboratory, the Accelerator Division and the CDF and DZero collaborations for the outstanding performance of the Tevatron and the detectors. The experiments continue to produce a broad range of exciting results. The Committee strongly supports operation of the Tevatron in 2011. The projected integrated luminosity is up to 12 fb^{-1} and will double the existing data set. For a Standard-Model-like Higgs boson, this will either give the possibility of excluding the full mass region favored by the electroweak data or a probability up to 50% of a 3 sigma evidence in the difficult mass region around 115 GeV. It is worthwhile to leave the possibility open for operating the Tevatron in 2012 should compelling circumstances materialize.

The LHC is the next step at the energy frontier. Fermilab played a major role in the construction of the LHC and CMS detector, and is now making significant contributions towards commissioning and preparation for data analysis. The Committee supports the strong involvement of Fermilab in the present CMS experiment, and in the upgrade of the LHC accelerator and CMS.

The Committee reiterates the importance of Fermilab remaining aligned with the Particle Physics Project Prioritization Panel (P5) recommendation that "the US should continue to participate in the international R&D program for the ILC to position the US for an important role should the ILC be the choice of the international community". Therefore, the Committee supports the Laboratory's continued involvement in R&D for the ILC and its associated detector technologies. If an e^+e^- collider at the energy frontier is built, the Laboratory should be a major player.

A muon collider is part of the strategic plan of the Laboratory. Fermilab is a leading player in the world effort on the R&D for such a challenging and far-reaching machine. The physics program of a muon collider needs to be revisited and further developed with realistic detector configurations. Comparisons to the physics program of a multi-TeV e^+e^- accelerator need to be performed. The Committee therefore supports the Laboratory's plans for a physics and detector workshop to be held this fall. A presentation to the PAC of the plans towards demonstrating the feasibility of a muon collider, and new ideas and initiatives towards solving the challenges of this machine would be valuable.

Fermilab should continue to explore the possibility of hosting the next machine at the energy frontier, whatever the machine may be.

Project X (Accelerator Design)

Project X is central to Fermilab's strategy for the future development of the accelerator complex, providing an evolutionary path to the energy and intensity frontiers. P5 recognized the importance of a multi-MW proton source based on a neutrino beam for a Long Baseline Neutrino Experiment (LBNE) as an essential ingredient in the exploration of the intensity frontier. This need is met by Project X. Project X simultaneously enables the search for lepton-flavor violation and high-precision kaon experiments, also recognized by P5 as high-priority activities.

The original Initial Configuration Document (ICD-1) consists of >2MW of beam power from the Main Injector over the range 60 – 120 GeV; simultaneous with >150 kW of beam power at 8 GeV; and compatible with a future upgrade to 2-4 MW at 8 GeV. The Laboratory's Project X technically-limited schedule was presented with CD-0 in 2009, CD-1 in 2011, CD-2/3a in 2012, CD-3 (initiate construction) in 2013 and a five-year construction period. A project management team has been established. There is a provisional site for the ICD-1 design. High-level design criteria have been defined and a preliminary cost estimate has been prepared. Both the design criteria and cost estimate have had a Director's Review. A multi-institutional international collaboration is being formed. The Committee is encouraged by the progress towards a Project X CD-0 coordinated with LBNE and a mu2e experiment by the end of 2009.

The primary goal of the Project X Research, Design, and Development (RD&D) program is to complete a fully developed baseline scope, cost estimate, and schedule in 2012 (CD-2), while retaining alignment of Project X to the ILC and the Neutrino Factory/muon collider programs.

Project X shares 1.3 GHz technology with the ILC. Project X requires ILC-like cryomodules, however the operating conditions are somewhat different. For CD-2 it is important to demonstrate the feasibility of Project X cryomodules. The cryomodule production assembly plan was presented to the PAC. The fabrication and testing of the Project X preliminary cryomodules is scheduled for 2011. Project X and the ILC would benefit from additional resources being made available to enable the test to be brought forward in time.

Another configuration (ICD-2) was presented to the PAC. In developing accelerator designs, it is important to retain as much alignment as possible with the ILC/muon collider.

The Committee commends Project X management on plans for regular reviews of the accelerator design by international committees of accelerator experts, and commends the Directorate for its plans for a Project X workshop linked to a muon collider workshop in the fall of 2009 as a way to build the Project X accelerator community and the muon collider community.

Project X and ICD-1&2

The broad research program of Project X described in the "Golden Book" includes long-baseline neutrino experiments, neutrino interaction experiments, ultra-rare muon and kaon decay experiments, quark flavor experiments, and experiments with antiprotons. The Committee strongly recommends that Fermilab include budget estimates for the highest priority projects, such as muon-to-electron-conversion, $K^+ \to \pi^+ \nu\bar{\nu}$, and $K_L^0 \to \pi^0 \nu\bar{\nu}$ experiments, in the budgetary planning for Project X in order to insure a broad initial program of high-impact intensity frontier physics.

The long-baseline neutrino experiments and rare-decay experiments benefit most directly from the high beam power afforded by Project X. Although the design of the Project X accelerator complex described in ICD-1 provides for the beam power required for the long-baseline neutrino program, the high beam power produced at 8 GeV is not readily available for a full rare-processes program. As a result, the accelerator group is considering ICD-2 based on a 2 GeV CW linac containing some ILC-like modules, providing both higher beam power and a more flexible beam distribution scenario for the rare-processes program. This would be followed by a 2-8 GeV accelerating section similar to the SC linac of ICD-1 or a rapid-cycling synchrotron. This combination can provide great prospects for satisfying the broad goals of Project X. The ICD-2 can drive the long-baseline neutrino program while also providing nearly ideal beams for the important Project X rare-processes research program.

The ICD-2 concept is extremely promising. The Committee strongly endorses further study of the prospects to realize the great potential of the high-sensitivity studies of rare muon and kaon processes which will be essential elements of the intensity frontier explorations of new physics at high mass scales. The Committee strongly encourages Fermilab to further develop the accelerator aspects of this proposal, including cost considerations, and to continue to consider the synergy with the ILC linac technology. The Committee supports further detailed studies of the ICD-2 kaon and muon beams to confirm their suitability for next generation experiments.

Mid-term Kaon Program

The Committee reiterates its view that a high-statistics, on the order of 1000 events, $K^+ \to \pi^+ \nu\bar{\nu}$ experiment represents very compelling science. While this was previously thought to be achievable only with the high power of Project X, the Committee was excited to hear about the prospects for such an experiment using the existing combination of the Main Injector and the Tevatron, operated as a stretcher ring, employing the well-studied techniques developed at Brookhaven National Laboratory where the initial observation of $K^+ \to \pi^+ \nu\bar{\nu}$ was made. Such a high-priority experiment would considerably strengthen Fermilab's intensity frontier program in advance of Project X.

The Committee strongly recommends that Fermilab evaluate the cost and feasibility of various options for making the Tevatron available for the $K^+ \to \pi^+ \nu\bar{\nu}$ and possibly other experiments. Should the Laboratory decide to proceed with this program, the Laboratory should ensure that the timeline is internationally competitive.

In light of the physics interest in rare-kaon experiments, it would be valuable for Fermilab to explore the full physics potential of the 120 GeV Main Injector and Tevatron stretcher opportunity as part of their *Physics with a High Intensity Proton Source* workshop series.

Future Long-Baseline Neutrino Physics Program

Lessons Learned from NuMI

The Committee commends the Laboratory for carefully assessing the lessons learned from building the NuMI beamline. The experience gained should position the Laboratory well for the major civil construction project for a beamline for LBNE. The two projects share the upstream part of the beamline. However, LBNE calls for a somewhat shorter decay region and a deeper near detector compared to NuMI. Nevertheless, the scale of civil construction for the two projects is very similar, which makes the lessons learned from NuMI applicable.

A complete design team from the beginning of the project is essential. Also, it is important to understand the beam parameters early in the project. Adequate costing with ample contingency is needed to cover the cost from unforeseen problems during the civil construction. Sufficient allowance for spares is important to avoid unnecessary extra costs and down time. To ensure smooth detector operations, it is vital to keep the detector construction specialists through the commissioning, startup, and debugging process. Technical reviews by expert panels proved to be very useful previously, and should be scheduled frequently. A close relationship with the subcontractors and outreach to local communities are important. Given the overlapping in time of NuMI running, NOνA construction and operation, and LBNE work, the Committee is concerned about the potential deficit of human resources available for these efforts.

Long Baseline Neutrino Experiment (LBNE)

Measurements of neutrino mass and CP violation are fundamental to understanding physics beyond the Standard Model, and have profound consequences for cosmology. Building on the unique infrastructure of Fermilab, combined with the proposed DUSEL, the community is developing a world-leading long-baseline neutrino experiment that addresses the measurement of the mixing parameter θ_{13}, the mass hierarchy, and CP violation in the neutrino sector as its principal goals. The experiment also provides new capabilities to search for proton decay and to observe neutrinos from astrophysical sources. The community has identified two modular detector technologies that can achieve these goals: a 3×100 kTon (fiducial) Water Cerenkov detector and a 3×17 kTon (fiducial) LAr TPC detector.

The Water Cerenkov technique is well-established. The LAr TPC is a less established, but promising technique. The feasibility of scaling up both technologies to the size needed for a far detector module is being actively studied. For a Water Cerenkov detector, the issues include photomultiplier procurement and cost. For a LAr-based detector, the issues include technical risk, cost and safety. There is a well-defined LAr R&D program in the US involving the construction and study of successively larger LAr TPCs. This program is centered at Fermilab, it has been previously endorsed by the PAC, and is progressing well. The Committee finds that Fermilab is playing a leading role in the development of LAr TPC technology and is pleased to see the first events recorded by the 0.3 Ton ArgoNeuT.

A plan to coordinate activities between DOE- and NSF-supported activities for DUSEL has been presented. The Committee commends this coordination. The DOE has charged Brookhaven National Laboratory and Fermilab to work together to provide materials to support LBNE CD-0, and then CD-1 by the end of 2010. Funding in the amount of $15M from the American Recovery and Reinvestment Act has been made available to speed this process. Fermilab responsibilities are project management and beam; Brookhaven has responsibility for the far detector. Mission-need documentation for CD-0 has been filed with the DOE, and is under review. Project management teams at Fermilab and Brookhaven are being staffed. A plan to produce the documentation required for CD-1 has been developed, and was presented to the PAC. (CD-1 does not require a technology choice.) The timeline is aggressive. The plan includes making comparisons of the detector performance using common tools and GEANT4 simulations. The Committee is encouraged by this approach, which it views as essential to an accurate comparative assessment of the strengths of the two technologies. The plan also includes making a comparison of the costs of the two technologies using the same methodology, focusing on cost drivers for the far detector determined by design teams. Data will be submitted to the project office which will determine the construction cost of a single module, and eventually of three modules. The initial cost estimate for the neutrino beam conventional construction and a strawman design for a near detector will be evaluated as well.

The Committee finds that Fermilab and the LBNE collaboration are making substantial progress in the beam design and project management.

The Committee recommends that frequent internal reviews of the beamline and detectors be staffed by independent experts drawn from the international community.

The Committee further recommends that a process and timeline be developed that will determine the final far detector configuration and technology to be decided for CD-2.

The Committee was pleased to receive the report of the Fermilab Water Cerenkov Design Task Force. Given the importance of the LBNE experiment to both the international community and Fermilab, the Laboratory is encouraged to consider a significant role in the Water Cerenkov effort.

Fermilab Center for Particle Astrophysics (FCPA)

The FCPA Strategic Plan

The strategic plan for the Fermilab Center for Particle Astrophysics (FCPA) was presented. This is a vibrant program, with highly successful activities in four core areas:

- Theory
- Dark Energy with DES
- Dark Matter with CDMS and COUPP
- High-Energy Astrophysics with the Pierre Auger Observatory.

along with the continued important development of cosmological computing capabilities.

The Committee commends the efforts within the FCPA strategic plan toward defining decision criteria and mechanisms to ensure continued evolution in important directions. The Committee strongly supports the plan, and encourages further development with the following comments:

- The document describes well the considerations that should apply to programmatic decisions, but it would be useful to prioritize the criteria and sharpen the description of the selection process.
- Further strategic thinking is important. For example, an overall roadmap for the FCPA will help identify emerging opportunities and time periods in which significant new projects could be carried out. This roadmap should be integrated into the overall plan for the Laboratory. The roadmap would help identify and prepare for decision points and maximize opportunities both for the Center and for the Laboratory as a whole.
- As physicists approach Fermilab, in part to benefit from its expertise and its resources, it is important to involve the outside community and the expertise it offers when making decisions. For medium and large projects, for example, the same outside review process that Fermilab has applied successfully to other areas in the Laboratory should be followed.
- Visibility in the community should be further enhanced. For example, a large workshop could be held periodically, with the topic open to proposals from the

outside community. These workshops would be co-organized by outside community members and Center personnel.

Astrophysics Theory

The Fermilab Theoretical Astrophysics Group has a long history of outstanding research. The group is addressing concerns of the DOE theory review by increasing interactions with the Particle Physics Theory Group, and the Committee encourages these efforts.

Pierre Auger North

The Pierre Auger North project proposal is currently under review by the community, in both the PASAG and the Astro2010 Decadal Survey. If it is highly ranked and moves forward, the Committee would welcome hearing a detailed presentation at a future meeting about the plans for Fermilab involvement.

QUIET II

The Cosmic Microwave Background (CMB) provides unique information on fundamental physics such as dark matter, dark energy and inflation. Therefore, it has a strong overlap with the intellectual mission of the Laboratory. A CMB polarization experiment could be an excellent candidate for a broadening of the Center's scientific scope, could use unique Laboratory capabilities, and could lead to scientifically interesting results with a modest investment. Specifically, a possible role in the QUIET II project is under discussion, but the Committee has not yet seen any of the details. The Committee would welcome a presentation at an upcoming meeting, including an evaluation of the different techniques, the technical status and risks of the project, the roles of the Laboratory, and the reasons why QUIET II is the right choice among the possible projects in this area.

Joint Dark Energy Mission (JDEM)

The science that would be enabled by JDEM is compelling, and the Science Operations Center is an appropriate role for Fermilab. Hosting the Science Operations Center would both put the expertise of the Laboratory at the service of the community and provide an opportunity to place Fermilab scientists at the core of the scientific analysis. However, there is still considerable uncertainty about the future of the mission and its final capabilities. In this atmosphere, Fermilab should not only continue to develop the Science Operations Center concept, but also take a leading role in defining, with the community, the next stage in the dark energy area.

P-990 Holographic Interferometer (C. Hogan)

The Committee considered the suggestion that the holographic principle, or at least one interpretation of it, could be tested in the laboratory. A pair of laser interferometers may be able to measure the predicted spatial fluctuations and disentangle them from other noise sources. If true, this would open a unique window onto the Planck scale. This fits squarely in the intellectual mission of the Laboratory. Moreover the transfer of optical cavity expertise to Fermilab may be important for other Laboratory experiments (e.g., for a future axion experiment).

The burden of proof is as high as the potential opportunity is exciting. The Committee therefore supports a rapid and targeted development of the proposed concept with two complementary goals:

- Build a broader understanding in the theoretical and experimental community of the soundness of this approach and of the significance of experimental results. Questions that should be widely addressed include: How generic is the prediction? Is the idea already excluded by other constraints? What would we learn from a negative result? Can the effect be excluded at GEO600 in the near future? What sensitivity goals should be pursued in a more general framework?

- Through a critical review with external experts (both theorists and experimentalists), establish the feasibility of the proposed experiment to provide definitive results. Among other issues, an important design challenge is to ensure that common-mode noise between two close-by interferometers would be under control.

After the initial work is done successfully toward the first goal, the Committee supports modest investments to bring this concept to a design level where the cost and risk can be evaluated. The Committee suggests increasing technical collaboration with GEO600 (e.g. with visits by Laboratory experimentalists) and more generally with the large interferometers.

P-986 Medium-Energy Antiproton Physics Experiment (D. Kaplan)

The Committee thanks the P-986 Collaboration for supplying additional information on the response and physics reach of their proposed experiment which would take advantage of the Fermilab Antiproton Source to provide an antiproton beam impinging on a gas jet. The described physics of charm and the X(3872) resonance accessible by a medium-energy antiproton experiment is interesting. However, it is still limited in scope, given the prospects of similar physics opportunities potentially better addressed in broader programs of both existing (Tevatron, LHCb, BES III) *and* planned future (SuperB and SuperKEKB) facilities. The case presented is not strong enough to warrant the investment required.

P-975 NuSONG (J. Conrad and P. Fisher)

The Committee received additional material sent to the Laboratory detailing the potential capabilities of the NuSOnG experiment to explore low-energy QCD. However, the Committee chose not to revisit its previous two recommendations that because NuSOnG would be a major effort and does not fit within the Laboratory's strategic plan for the coming decade, it should not receive Laboratory resources.

BooNE Expression of Interest (W. Louis)

The Committee received an Expression of Interest (EOI) sent to the Laboratory by the BooNE collaboration to build an additional detector to be positioned at half the baseline of the current MiniBooNE detector (or to move the current detector to that location). Given that the Laboratory approved an additional 5×10^{20} protons-on-target following the PAC's recommendation at its last meeting, the Committee recommends addressing this EOI after additional results from the antineutrino data are available.

~1-TeV Tevatron Fixed-Target Program (K. Arms and A. Schwartz)

The Committee has received a white paper sent to the Laboratory detailing physics opportunities of a possible TeV-scale fixed-target program, including charm and neutrino physics. This program would require retaining the Tevatron experimental capabilities after the end of the collider program. An ~1 TeV fixed-target program typically involves large detectors, and hence is a major commitment of resources.